

CORRESPONDENCE.

Habits of the Great Crested Grebe.

EDITOR OF 'THE AUK'

Dear Sir:—

In this year's (1916) April number of 'The Auk,' Mr. Julian Huxley, in his interesting paper 'Bird Watching and Biological Science,' says, speaking of the Great Crested Grebe:—"There (that is to say in inland waters) in February, pairing-up takes place, a process not yet wholly disentangled, but certainly associated with a great deal of flying and chasing" (p. 150). Insofar, however, as I have been able to observe, this supposed pairing-up process does not take place at all, so that there is nothing to disentangle in relation to it, nor do any difficulties, specially appertaining to the behavior of the birds at this time, present themselves. Mr. Huxley was kind enough, before he left England, to send me his notes upon this species, and he suggested that I should investigate what took place immediately after the arrival of the birds in February, since he himself was precluded from doing so.

Accordingly, on the 15th of February, 1915, I went down to the Fring Reservoirs, and was told, by the keeper at whose cottage I stayed, that only two birds had yet been seen anywhere. Next day, however, the head keeper sent word that six had come down (I think the evening before, *i. e.* the 15th) on one of the two larger sheets of water. It was the opinion of the keepers that my own arrival and that of the birds synchronised closely. From now onwards, I watched the birds, up to March the 7th, by which time most, if not all of them, had at least located their nesting sites. As a result, I can say that, according to what I saw, these Grebes (I am not considering young and previously unmated birds, of which there was no indication) arrive paired, that they enter, either at once or very shortly, upon their conjugal display actions, and that the flying and chasing is neither a very pronounced feature, nor has it the import which has been attributed to it by (if I mistake not) the head keeper of the Tring Reservoirs; that is to say, it has not essentially to do with the assumed pairing-up of the actually already paired birds. The above is the gist of the notes which I took, and which still remain in their MS. state. Otherwise I should have sent them to Mr. Huxley, and had, indeed, intended to do so, in any shape or form, but one thing gets in the way of another, and tardiness increases with age.

It would seem therefore that, as I had suspected (and suspect in many more instances than where this is supposed to be the case) the Great Crested Grebe pairs for life, which fact, if established, would be in harmony with my view that excitatory sexual movements either first arose under,

or continued after, the first union of the sexes, to accompany monogamous conjugal relations, and then, by a process of evolution, the steps in which may, I think, be partly traced and partly inferred, passed, as a culmination, into true Darwinian sexual selection. I do not however mean to imply that this has been the invariable course of development, or that mere promiscuity may not, at an earlier stage, have sometimes preceded monogamy.

At p. 151, speaking of the "ceremonies connected with coition," Huxley says:— "The chief point to be remarked is that both cock and hen may adopt this attitude" — that is to say the prostrate attitude, preceding and accompanying coition, which rightly belongs to the female alone. My own observations, however, made in 1900 and 1901, were sufficient to assure me that this interchangeability of action as between the two sexes, in their sexual relations, extended to the actual pairing itself, and I have since confirmed this in the case of the Little Grebe (or Dabehick), for, having closely and continuously watched a pair of these birds, established in a pond, and thus, as I may say, well under control, I have seen either bird alternately assume the part of either sex during coition. This reversal extended to the minutest particular, so that the false and true unions were indistinguishable. Thus we have — for what else are we to term it? — functional hermaphroditism in both the Great Crested and Little Grebe. My observations on the latter species were published in 'Wild Life' from July to December (inclusive), 1915.

It is, I think, a legitimate inference that this dual functioning of either sex, in the primary and all-important sexual act, must (or is likely to) imply a similar duality of the sexual psychology, in each, and this would, in itself, account, or help in accounting for, the identity of much of the masculine and feminine conjugal display action in the Great Crested Grebe. I have made similar observations on the Moorhen — in which species also this identity exists — and, so far as the actual pairing is concerned, in the case of the Dovecote Pigeon. Also I have good first hand evidence of the same nature concerning the Mute Swan, and can myself speak as to very salient springtide antics carried out by both the male and female Whooper Swan, when conjugally united. To me it is almost inconceivable that these peculiar pairing habits have been brought about, independently, in different species, through the operation of more or less recent utilitarian causes. The root cause is, I believe, the joint inheritance, by all, and in each sex, through a common line of ancestry, dating from a remote past, of that sexual psychology which once co-existed with physiological hermaphroditism; of which persistence, therefore, the lesser or secondary bisexual activities are also to be regarded as effects. It is, of course, obvious that, so far as the sexual mentality of birds is concerned, the above inference need not alone apply to species that have this odd habit of double coital functioning, for a general inherited tendency need not necessarily be accompanied by some particularly salient indication of it, in action. The study of man sufficiently illustrates this.

In the paper in which I first recorded the activity here specially dwelt upon, in the case of the Great Crested Grebe, I put forward the above view, in explanation of it. Now, many years afterwards, I learn that the late Professor Metchnikoff held the same opinion (whether in reference to my own notes which, so far as I know, first placed the facts upon record, or otherwise, I am not sure) and Haeckel's concurrence also, I think, lies implicit in his work 'The Evolution of Man,' though he does not there mention — probably through not having been aware of it — the matter in question. I would suggest, therefore, under shelter of these names, that a new possible factor enters into the philosophy of nuptial or ante-nuptial excitatory actions in birds, and, through these, of true purposive display and progressive sexual selection.

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NOTES AND NEWS.

PROFESSOR FOSTER ELLENBOROUGH LASCELLES BEAL, a Fellow of the American Ornithologists' Union, died suddenly at his home near Berwyn, Md., October 1, 1916. Professor Beal was in the 77th year of his life and in the 25th of service in the U. S. Biological Survey. He was born at South Groton, Massachusetts, January 9, 1840. His early life was spent upon a farm, but he was determined to get an education and was graduated from the Massachusetts Institute of Technology in 1872. He was professor of mathematics in the United States Naval Academy in 1873-4, and professor, in turn, of mathematics, zoölogy, and geology in the Agricultural College at Ames, Iowa, from 1876 to 1883. He was employed in the Biological Survey for six months in 1886 and began his permanent term of service in 1892. He prepared, either wholly or in part, 24 official publications, besides numerous other scientific articles, and played an important part in building up the existing system of laws for the protection of American birds. A full account of the life and work of Professor Beal will be published in a later number of 'The Auk.' — W. L. M.
